Department of Petroleum Engineering
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-5215

January 12, 1980

Dr. Robert Schiffman
Department of Civil Engineering
University of Colorado
Boulder, Colorado 80607

Dear Bob:

I have completed my review of the work done by Dr. Louis Thompson and his students on the U.S. Geological Survey sponsored research project on overpressured submarine sediments. As you requested, emphasis was placed on reviewing (1) Dr. Thompson's test procedure and test data and (2) the potential technology which can be developed by this project.

The stated purpose of Dr. Thompson's research is to develop a sediment consolidation theory applicable to the progressive burial of ocean bottom sediments which will predict the porosity of the sediments as a function of depth and time for various rates of deposition on the ocean bottom and various mineral compositions. The U.S. Geological Survey is interested in supporting this research because of its potential application in the prediction of formation pore pressure and hydrofracture pressure in shallow marine sediments during offshore drilling operations.

A pore pressure predictive techinque in sediments having a depth of burial less than the depth at which surface casing is usually cemented in place is particularly important because prior to that time a well cannot be safely closed by the surface blowout preventer valves without risk of hydrofracture through the shallow sediments to the ocean floor and total loss of control. A hydrofracture predictive technique is also important in determining the optimum depth of the surface casing. At present, the U.S. Geological Survey usually requires that surface casing be cemented in place prior to penetrating more than 4500 ft into the sediments. The placement of additional deeper casing strings is also of great economic significance to the well operator, but is not as important from an environmental standpoint. Also, Dr. Thompson's work is directed more towards the shallow sediments where the diagenetic processes at work are more easily modeled in the laboratory.

The written documents submitted for review by Dr. Thompson included:

1. a summary document of recent work entitled "Soil and Rock Mineral Contact Areas Revisited"

The formation pore pressure and hydrofracture pressure as a function of well depth, are probably the most critical parameters governing the design and cost of abnormally pressured wells. Because these parameters are so critical to a successful well design, a large amount of previous work has been done on emperical prediction methods. I have attached a selected bibliography (draft) prepared by an API subcommittee on this subject to give you an idea of the scope of the previous work in this area. Most of the previous investigators, like Dr. Thompson, have proposed emperical predictive techniques which are based on a porosity dependent parameter measured in a given formation type (usually shale). It has been my experience that none of these techniques work with a high degree of reliability or precision and there is certainly room for improvement.

Truly yours,

Adam T Bourgoyne Jr

A. T. Bourgoyne, Jr., Chairman
Petroleum Engineering Department

ATB/mhh

Enclosure

COMMENTS ON THE RESEARCH PROJECT OF DR. LOUIS J. THOMPSON, TEXAS A&M UNIVERSITY, ENTITLED "OVERPRESSURED MARINE SEDIMENTS"

The purpose for this research, as noted by Dr. Thompson, is to develop a marine sediment consolidation theory for prediction of porosity and temperature in sediments as a function of depth and time, with application to prediction of blowout potential while drilling. John Gregory of the U.S. Geological Survey, program manager for this research project, indicated that his prime interest was directed toward the stated application.

A major question in my mind concerns the application of the research to sedimentary rocks in which diagenesis is an important factor. Both the consolidation and the energy equations in the report by Dr. Thompson apply to sediments; cohesion is included in the various empirical coefficients. However, the added rigidity of the sediment framework due to cementation and recrystallization may require an entirely different analytical approach to the problem of prediction of overpressured rock formations. It seems timely to sample some actual shales from drill cores for testing as suggested during the discussion at the meeting of the review group at the U.S. Geological Survey in Reston, Virginia, on 6 November 1980. The removal of shales from deep in situ conditions may cause fracturing or other failure in the samples due to dilatation that would prevent recovery of the in situ physical properties such as porosity.

My general conclusion is that, although this research provides an important contribution to marine sediment consolidation studies, I do not see a clear path from the presently reported progress to an ability for prediction of deep drill hole blowout potential in sedimentary rocks. The project has greater relevance to problems of bottom stability that have caused several drilling platform disasters in the past, and it should provide another useful, but not unique, method for prediction of blowout potential in shallow drill holes (depths below the bottom on the order of hundreds of meters). Several members of the review group also pointed out the value to their programs of much of the data already generated by this research, indicating the relevance to petroleum engineers for purposes other than blowout potential prediction. Because porosity is closely related to seismic wave speed in sediments, I believe that borehole studies of the seismic speed ratios (Vp/Vs) and high frequency acoustic propagation offer greater potential for prediction of in situ conditions in sedimentary basins. Western Geophysical Company has been a leader in this field.

Finally, the results of the research to this time should be submitted for publication to elicit a broad peer review.

Robert S. Andrews

Robert S. Andrews

THE JOHNS HOPKINS UNIVERSITY · BALTIMORE, MARYLAND 21218

J. L. ERICKSEN
Professor of Theoretical Mechanics
Latrobe 122

November 26, 1980

Dr. Robert Schiffman
Department of Civil Engineering
University of Colorado
Boulder, Colorado 80309

Dear Bob:

There are various ways of thinking about, or decomposing stresses in composite materials. Professor Morita described three different notions of effective stress, none of which seem to jibe with Thompson's views. Clearly, we can't cope with the complicated variations occurring within grains, etc., so we must deal with some kind of averages. Some interested in composites like to use volume averages, so I will try to put these in perspective. Of course, the idea is to average over a volume V, relatively large compared to grain size, so that porosities, etc. have meaning. On a finer scale, we have a stress tensor, σ_{ij}, varying in a complicated way, discontinuous at surfaces where water contacts a mineral grain, or where two grains are in contact. For simplicity, I will not consider body forces. Where it is smooth, the stress should satisfy the usual equilibrium equations

$$\sigma_{ik,k} = 0.$$

At the discontinuity surfaces, we commonly assume that the two limiting values σ^+_{ij} and σ^-_{ik} give balancing forces. That is, if ν_k is the unit normal

$$(\sigma^+_{ik} - \sigma^-_{ik})\nu_k = 0.$$

Then, pick any volume V. The volume averaged stress is given by

$$\langle\sigma_{ik}\rangle = \int_V \sigma_{ik} dv/V = \int (x_k \sigma_{ij})_{,j} dv/V = \oint x_k \sigma_{ij} dS_j/V$$

comma

where dS_j is the vector element of area. Having picked V, we will have

$$V = V_m + V_w,$$

where V_m is the volume occupied by mineral, V_w the water volume. By similarly averaging over these subvolumes, we can define average mineral and water stresses,

$$<\sigma^{m}_{ik}> = \int_{V_m} \sigma_{ik}/V_m, \quad <\sigma^{w}_{ik}> = \int_{V_w} \sigma_{ik}/V_w,$$

and we have

$$* \quad <\sigma_{ik}> = \frac{V_m}{V} <\sigma^{m}_{i}k) + \frac{V_w}{V} <\sigma^{w}_{ik}> = (1-n) <\sigma^{m}_{ik}> + n <\sigma^{w}_{ik}> ,$$

where

$$n = V_w/V$$

in the porosity. Following the notation and sign convention of Thompson, we expect that, in the water, $\sigma_{ik} = u\delta_{ik}$, where u is the pore pressure, giving,

$$<\sigma^{w}_{ik}> = u\delta_{ik}.$$

Of course, it is much harder to assess the mineral average. Theorists will introduce linear elasticity, or some other theory of solids for the grains, and try to use it to make some estimate. Or, they might proceed more directly, to propose some constitutive equations for it. In the above view, grain-grain contacts are generaly inside V_m. The part of the boundary of V_m that lies inside V is then loaded by pore pressure. The remainder forms part of the boundary of V, the rest of the latter being water. If the entire boundary of V_m were loaded by the pore pressure, we calculate that

$$<\sigma^{m}_{ik}> = u\delta_{ik},$$

giving

$$<\sigma_{ik}> = u\delta_{ik}.$$

I would think that a sizeable part of its area would be so loaded, so it does not seem reasonable to think that the mineral stress is not influenced by pore pressure. At least roughly, this is what Thompson assumes, to get his measurements of area ratios. Thus, I share Professor Moritas reservations about this.

Clearly, the boundary of V is not to be considered as subject to the pore pressure alone. We could think of the loading as being described as a pore pressure, plus some excess over this, giving

$$<\sigma^m_{ik}> = u\delta_{ik} + \bar{\sigma}_{ik} ,$$

where $\bar{\sigma}_{ik}$ arises from the excess loading. Putting this altogether gives

$$** \quad <\sigma_{ik}> = (1-n)\, \bar{\sigma}_{ik} + u\delta_{ik} ,$$

which begins to look like the effective stress idea, with $(1-n)\bar{\sigma}_{ik}$ interpreted as effective stress.

Formally, * resembles Equation (5) in Thompson's final report, seeming to coincide with it, of E = 1. He opines that the assumption E = 1 is reasonable for clay or shales ,but not sands. This would seem to indicate that either he is wrong, or that it is inappropriate to use our volume averages, as measures of partial stresses. At our meeting, we agreed, I think, that it is not feasible to directly measure mineral stresses. To directly measure area ratios, it would seem that the only hope would be to freeze and section specimens, then try to estimate these fractions. Experts seemed not to be optimistic about accomplishing this. Thus, it seems hard to determine, conclusively, whether he is right or wrong about this. One might like to deal with intergranular stresses and the control areas separately, so there is room for debates. However, we might be dealing with red herrings.

What he starts doing is to consider loading on plane surfaces. For his discussion to make sense, his area ratios must be interpreted as some kind of averages. Probably, he has in mind averaging over areas, of dimension large compared to typical pore dimensions, <u>etc.</u>, and the forces are similarly averaged. It is not very easy to compare such area averages with volume averages. One thing seems worth noting. We can take V to be, say a cube, with side area a. As estimated from the volume-averaged stress, the force on a side x_3 = const. will be

$$F_i = <\sigma_{i3}>a = (1-n)\, <\sigma^m_{i3}>a + nu\delta_{i3}a.$$

This comes into line with the area averaging if na is the water area, and not otherwise. Granted that area ratios can be different on differently oriented planes, it is not so easy to see how to define a bulk stress better matching the area averages; I don't find palatable Thompson's ideas on such three-dimensional theory. It is a bit worrisome that volume averaging fits well only when the water area ratio is n. I don't have a good feeling for how bad the assumption is, or what real difficulties we might get into, by using such a fictional value. I don't accept Thompsons values, since I don't accept assumptions required to get these, from indirect measurements.

Basically, heat flux h_i should be treated in the same way as stress, $h_i dS_i$ giving heat flow through a surface. Looking at his Equation 87, I would expect to see $\partial h_w/\partial z$ multiplied by n^E, for example, if he were to apply consistent reasoning. Instead, he is, unwittingly using ideas more like those associated with the above volume averaging. He would get into a real mess, if he tried to use the area averaging ideas. Again, we have consistency if E = 1, and not otherwise. For the clays and shales, he takes E = 1, so we then have internal consistency.

His discussion emphasizes the importance of those area rations. I could understand his Equation 10 without being concerned about them, writing

$$\sigma = un^E + An^B$$

so, OK, this how the total stress happens to depend on u and n. It is a separate question to explain why, or to relate E to an area ratio. As indicated above, we might find it convenient, for theoretical purposes, to introduce some decomposition, the "volume average" decomposition being common. In itself, it dictates a kind of area decomposition, which might or might not be close to what actually occurs. Show me some hard evidence that it is far off, and I will begin to worry seriously about the foundations of such theory, including some of Thompson's theory. From this point of view, it is important to know whether his measurements of area ratios can be trusted, and I seriously doubt it. Really, I don't know what else to say, except that I concur with your view of the desirability of publication. No doubt, the data are useful to others, and critical reviewing could have helped him, as well as others.

Best Wishes,

Jerry

J. L. Ericksen

JLE/cm

P.S. Concerning the possible mid-December meeting, I will be tired up in another meeting, December 14-17, in Atlanta.

I meant tied, but this was too good to erase

REPORT ON RESEARCH BEING CONDUCTED BY DR. LOUIS THOMPSON ON OVERPRESSURED MARINE SEDIMENTS

The panel listed below was convened by Mr. John Gregory for the purpose of assessing the research being conducted by Dr. Louis Thompson under contract to the U.S. Geological Survey (USGS). The panel met at USGS Headquarters on November 6, 1980, at which time Dr. Thompson made a presentation concerning his work. The panel next met in Boulder, Colorado, on January 6, 1981, at which time a draft report was formulated and circulated to the panel. In the interim, members of the panel presented reports and commentary. These are attached. On December 14, 1980, two members of the panel (Olsen and Schiffman) briefly visited Dr. Thompson at Texas A & M University and observed some aspects of the experimental work.

This report represents a concsensus opinion of Dr. Thompson's research.

The panel recognizes that some aspects of Dr. Thompson's research are controversial. It is to these areas that this report is primarily addressed. These are:

1. The analysis of effective stresses.
2. The contention that the pore water pressures in marine sediments can substantially exceed the geostatic pressure.
3. Test procedures and test data.
4. The technology which is likely to result from the project.

Analysis of Effective Stresses

Dr. Thompson's thesis is that the effective stress principle as formulated by Terzaghi is incorrect in that it assumes a negligible mineral contact area. Dr. Thompson further bases his analysis on a measurement or interpretation of relative contact areas of mineral and water. In this analysis, Dr. Thompson expresses a viewpoint which takes a position which is contrary to established engineering and scientific opinion. The issue concerning the influence of mineral contact areas was specifically discussed by Skempton[1]. Prof. Skempton specifically states in analyzing compressibility:

> "The foregoing experiments, together with the unjacketed compressibility tests on Marble and Quartzite, therefore confirm that the effective stress controlling volume changes in porous materials is given with sufficient accuracy by the equation
>
> $$\Delta p' = \Delta p - \left(1 - \frac{C_s}{C}\right)\Delta u \ .$$
>
> And since this expression does not include the contact-area ratio, it follows that this parameter cannot be determined from volume change tests."

Prof. Skempton further summarizes the evidence with the statement:

> "It is usually assumed that the effective stress controlling changes in shear strength and volume, in saturated porous materials, is given by the equation
>
> $$\sigma' = \sigma - (1 - a)u_w \ , \qquad (1)$$
>
> where a is the area of contact between particles, per unit gross area of the material. Experimental evidence is presented however, which shows that equation 1 is not valid; and theoretical reasoning leads to the conclusion that more correct expressions for effective stress in fully saturated materials are

[1]Skempton, A.W. (1961), "Effective Stress in Soils, Concrete and Rocks," Pore Pressure and Suction in Soils, Butterworths, London, United Kingdom, pp. 4-16.

(i) for shear strength

$$\sigma' = \sigma - \left(1 - \frac{a\tan\psi}{\tan\phi'}\right) u_w \tag{2}$$

(ii) for volume change

$$\sigma' = \sigma - \left(1 - \frac{C_s}{C}\right) u_w \tag{3}$$

where ψ and C_s are the angle of intrinsic friction and the compressibility of the solid substance comprising the particles, and ϕ' and C are the angles of shearing resistance and the compressibility of the porous material."

The review panel has examined this question carefully and extensively. The attached reports by Bourgoyne, Ericksen, Morita and Gray, and Schiffman specifically address this issue.

The review panel is not convinced by the evidence presented by Dr. Thompson. We believe that Dr. Thompson seriously oversimplifies this complex behavioral problem. We are concerned that the oversimplification, if put into practice, seems likely to lead to misconceptions and potentially serious technological errors; for example, in the overestimation of pore pressures. If, on the other hand, Dr. Thompson is correct, the implications of his theory have far reaching practical importance. They could potentially revolutionize the field of geotechnical engineering.

We believe that it is of vital importance that Dr. Thompson document, justify and publish his results in publications which will expose this issue to review and discussion by his peers. We recommend one of the following journals:

Journal of Geophysical Research (Red)
ASCE Journal of the Geotechnical Engineering Division
Journal of Rock Mechanics
Geotechnique

One member of the panel (Powley) questioned the viability of publication at this time.

As an alternative to publication it is recommended that USGS sponsor a technical meeting with a limited number of invited qualified participants. The purpose of this meeting would be to review the science related to the effective stress principle.

Excessive Pore Pressures

Dr. Thompson contends that excess pore water pressures can be substantially greater than geostatic pressure. To the knowledge of this panel nobody has carefully documented this phenomenon. Current practice accepts the previous work by Skempton, Bishop and others which concludes that excess pore water pressure cannot exceed geostatic pressure in saturated materials. On the other hand, Dr. Thompson has stated that his laboratory experiments show excess pore water pressures which are considerably in excess of geostatic pressure. The area ratio data in the student theses prepared under Dr. Thompson's supervision carry the same implication.

Dr. Thompson's experiments if published would stimulate a professional re-examination of this phenomenon and would enhance the science. We therefore recommend, as before, that the experimental evidence be documented and submitted to peer reviewed journals which are open to professional discussion.

Procedures and Test Data

Dr. Thompson has produced a substantial amount of high pressure test data on permeability and compressibility, thermal effects, and the coefficient of earth pressure at rest (K_0). This data is in a range of pressures which are unusual and would be of substantial value to the engineering and geologic profession. We therefore recommend that this data, along with the test procedures, be published at the earliest possible time.

Technological Relevance

The goal of the USGS research program in this area is to improve the technology of blowout prevention. Dr. Thompson's research, if the science is correct, may lead towards this objective by other investigators. Unfortunately, the connection between the science and technological tools does not exist in a clear path. The attached comments by Andrews, Bourgoyne, and Powley address this issue.

The panel believes that the promise of this research program lies in providing quantitative and qualitative causal effects. To accomplish this it is emphasized that Dr. Thompson must have effective interaction with the practicing profession, which is usually achieved by frequent publication in the course of investigation.

Summary

In the four year period of this research project Dr. Thompson has produced two Master's theses with a wealth of valuable data and a controversial conception of the behavior of porous systems. We believe that the experimental data on permeability and compressibility, thermal effects, and (K_0) in the high pressure range would be a valuable asset to the profession.

We are concerned that Dr. Thompson's controversial views and analyses have not been exposed to the profession for publication and broad peer review. We recommend that this area of research be clarified before further work is done along these lines.

For the Review Panel,

Robert L. Schiffman
March 10, 1981

RLS/es

Attachments (8)

Review Panel

R.S. Andrews	N. Morita
A.T. Bourgoyne	V. Nacci
W.R. Bryant	H.W. Olsen
J.L. Ericksen	D.E. Powley
M.I. Esrig	R.L. Schiffman (Chairman)
K.E. Gray	W. Sweet

Amoco Production Company
4502 East 41st Street
P.O. Box 591
Tulsa, Oklahoma 74102
Research Center

November 26, 1980

80331ART0099

Mr. Robert L. Schiffman
Department of Civil Engineering
University of Colorado
Boulder, CO 80309

Dear Mr. Schiffman:

Subject: Review of Overpressured Submarine Sediments-Dr. Louis Thompson

Dr. Thompson's work is fundamentally different from previous work inasmuch as he is using reconstituted rock material whereas other workers, mainly in the petroleum industry, have used whole rock material as received at the surface, or used well logging-geophysical surveys of the rocks in place. Therefore, I feel inadequate to predict success or failure for Dr. Thompson's endeavor to correlate rock properties with subsurface pressures.

Dr. Thompson has established an excellent baseline of multistress consolidometer test data from reconstituted, normally pressured, shallow submarine sediments. He presumes that overpressured, shallow submarine sediments prepared and tested in the same manner will have a readily identifiable separate baseline. His study will be incomplete until he obtains some core material from an overpressured site. I am apprehensive that he will not get material to work with because most prudent drillers rigorously avoid coring in shallow, overpressured sections. I am further concerned that, even if Dr. Thompson obtains satisfactory material and establishes a separate baseline, he may not be able to obtain similar results with conventional well cuttings from shallow depths.

Because Dr. Thompson's work will not be conclusive until cores from an overpressured site are obtained, I recommend that further investigative work be postponed awaiting finding suitable cores. I further recommend that, during the interim, a report be prepared for publishing in some scientific journal outlining progress to date.

Very truly yours,

D. E. Powley

D. E. Powley

DEP:el
cc: T. Bourgoyne
R. S. Andrews

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Tulsa, Oklahoma 74102
Research Center

January 27, 1981

81027ART0027

Dr. R. L. Schiffman
Campus Box 428
University of Colorado
Boulder, CO 80309

Dear Dr. Schiffman:

Subject: Review of Research of Dr. Louis Thompson

I feel that the Review Panel report to John Gregory should point out that the controversies regarding effective stress and whether pore fluid pressures in marine sediments can substantially exceed the geostatic pressure, while of intense concern to his professional peers, may not be of immediate practical importance to attaining Dr. Thompson's original objective to develop an empirical relationship between consolidometer test responses of rock material from normally pressured and overpressured underground formations for use in recognition of overpressured formations when penetrated by wells. The comparison of empirical data cannot be done until consolidometer tests have been made on material from overpressured formations.

The development and defense of his theories regarding effective stress, etc., are somewhat peripheral to the USGS project objectives, but warrant discussion in recognized professional journals. Dr. Thompson may wish to defer defending his theories until the empirical observations are completed: in which case the review panel might be doing both Dr. Thompson and the profession a disservice by pushing for early publication. However, if Dr. Thompson cannot locate rock samples from overpressured formations, his views should be aired with the readers made aware of any theory development limitations imposed by the incompleteness of test data.

Sincerely,

D. E. Powley

D. E. Powley

DEP:sdg:pt